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The Cryosphere Discussions

Interactive comment on "A statistical permafrost distribution model for the European Alps" *by* L. Boeckli et al.

Anonymous Referee #3

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The authors present a new statistical model, or, better, a combination of models to predictpermafrost distribution of the European Alps. The main aims of the paper are not really well presented . The authors stated that the focus of the paper is the analysis of the explanatory variables, the developmentof the statistical sub-models and their combination but, both in the structure of the paper and in the title these aims are not truly developed in a clear way. The title, for example, seems more to suggest that the novelty of the paper is a new statistical permafrost distribution model for the whole European Alps, implicitly suggesting the presentation of one map with a calibration of the results.

Unfortunately this map is not presented and also the calibration of this permafrost distribution is practically absent.

The manuscript is not easily readable and in general it needs a shortening to avoid several repetitions especially on how the models work.

In particular, the paragraphs Introduction and Background can be shortened and included in a single paragraph (Introduction).

The main part of the statistical methods could be moved in an appendix. The references are not complete both regarding the methods as well as the permafrost models and generally the self-citations are too abundant and redundant.

However, the most important points regard the data set that the authors used for the models and on their calibration that require, at least, a critical discussion and not a simplistic assumption.

1) The choice to neglect GST, BTS, geophysical data and borehole temperatures is difficult to understand, also because the number of these data is quite high and useful to calibrate permafrost distribution .

2) It is not clear why the authors joined active and inactive rock glaciers in the category of the intact rock glacier. In many inventories (and may be also some inventories used by the authors) inactive rock glaciers are considered indicators of past permafrost distributionas the relict rock glaciers. Therefore the authors should at least explain the difference between inactive and relict rock glaciers and why the inactive rock glaciers should be included in the Intact rock glacier.

3) Rock glaciers should be used with care to calibrate permafrost distribution because is well known (as stated also by the authors) that their surface conditions can produce local perturbation of the thermal state of the ground, with important cooling, and therefore with a overestimation of the permafrost distribution.

4) It is not surprisingly that the probability of a rock glacier being intact is positively associated with increasingPRECIP because this reflects the possibility that they are debris rock glaciers. Indeed, debris rock glacier are very widespread in several parts

of the world, including European Alps. Despite of their origin, debris rock glacier are mostly developed in recently deglaciated areas or in the wetter areas of the Alps.

Personally, I think that the authors should test their models using only talus rock glaciers.

5) Regarding the MARST the authors should show more details of the data considered. Infact the depth of the sensors, their aspect and type of rock is not described as the year of the measurement. It is important to know these characteristics because the number of sensor is not so high and their distribution is localized only in some particular areas of the Alps and therefore this data set does not seem to be enough complete and robust to test all the investigated area.

6) It is also not clear why the authors need to adjust the MARST to longer term measurement and how they did this. Why they used Piz Corvatsch site and especially why they used the period 1961-1990? The period of 1961-1990 is surely not appropriate if the authors want to give an actual permafrost distribution because the warming trend was much more pronounced since 1990 to now.

7) The assumption that MARST follow MAAT is quite simplistic considering that is well known the effect of the radiation on the steep rock face.

8) The PISR was calculated for which year?

9) The Lapse rate of 0.65 °C/100 m could be not appropriate in several areas of the Alps and for several months in the year, please specify why they decided this lapse rate.

10) The size of the model for the precipitation 15 km seems not really appropriate considering the strong local variability of the precipitation in the Alps. There are some areas in which you can pass from 1500 mm/year to only 900mm/year in less than 15 km!

11) It is not clear which is the method used to distinguish the different surface types

and which is the accuracy of this method, please specify.

12) The three subset representing the different climatic conditions (drier-wet) should be clearly defined. In conclusion, I think that the manuscript is not acceptable in this form and that the authors could try to rewrite the paper with more care on the used data set and on the organization of the paper.

Interactive comment on The Cryosphere Discuss., 5, 1419, 2011.